tention. When lying quietly at rest, there was no apparent change in the respiration. No graphic records or measurements of the minute volume were taken. But when the dog was urged to rise and walk about, it at once began to pant violently. On lying down again, the panting ceased. Other dogs with normal cerebral circulation did not pant except after much greater exertion.

Hill states that none of his dogs died after ligation of the four cerebral arteries, but he does not mention the age of his dogs. I have found that vigorous, full-grown dogs survive the ligation indefinitely, but half-grown pups and old dogs usually succumb within twenty-four hours. I have seen half-grown pups lie unconscious for several hours, sometimes panting violently, and sometimes making ineffectual movements of locomotion with the fore limbs. Attempts to rouse them from this state were unsuccessful, and they were usually found dead the next morning.

Hill remarks that there must be a certain blood pressure resulting in the flow of a certain amount of blood through the medulla oblongata in order to provoke respiration. My experience tends to confirm Hill's conclusion. It is a striking thing to see an animal with failing respiration at a low blood pressure improve rapidly when the pressure is artificially increased.

In the dogs with restricted cerebral circulation, there was no apparent deficiency in the rest of the systemic circulation in those which recovered. Nor is there any reason to suppose that there was any change in the blood which would decrease its power of carrying either oxygen or carbon dioxide. It does not seem improbable that, in the dog with the marked respiratory disturbance, one would have found a somewhat greater concentration of oxygen and a somewhat lower concentration of carbon dioxide in the blood than in dogs with normal circulation. The condition in the medullary center itself, in which carbon dioxide might tend to accumulate in somewhat greater concentration than usual, would seem sufficient to account for the dyspnœa on moving about. A lower concentration of carbon dioxide in the blood would be the natural result of the forced respiration. In cases of shock resulting from abdominal wounds on the battle field, in which there was no deficiency of the systemic circulation prior to the wound, it does not seem necessary to assume the production of any large quantities of acid in the body to account for the lower concentration of carbon dioxide in the blood of such patients. It seems sufficient to suppose that, as the systemic blood pressure falls progressively lower, there would be a deficient blood supply to the respiratory mechanism in the medulla oblongata. The natural result would be an increase in the volume of respiration, and a decrease in the concentration of carbon dioxide in the blood. This would not in itself be a sufficient reason for postulating acidosis as a causative factor in the early stages of shock. Whatever acid might accumulate in the tissues might result, as Haldane[4] suggests, from the deficient supply of oxygen to the tissues.

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[4] *Loc. cit.*

ON THE HYDROLYSIS OF PROTEINS IN THE PRESENCE OF EXTRANEOUS MATERIALS AND ON THE ORIGIN AND NATURE OF THE "HUMIN" OF A PROTEIN HYDROLYSATE

In a recent paper McHargue[1] attempts to show that the nitrogen distribution of casein is not appreciably altered when hydrolyzed in the presence of five times its weight of starch providing that the hydrolysis is continued for only 12–15 hours. McHargue reaches a conclusion which is decidedly at variance with that reached by myself[2] and by Hart and Sure,[3] *i. e.*, that the presence of carbohydrates seriously vitiates the nitrogen distributions of a Van Slyke analysis and he explains the difference in the findings by his shorter hydrolysis. However, he makes several astonishing

[1] J. S. McHargue, *J. Agr. Res.*, Vol. 12, pp. 1–7 (1918).

[2] R. A. Gortner, *J. Biol. Chem.*, Vol. 26, pp. 177–204 (1916).

[3] E. B. Hart and B. Sure, *J. Biol. Chem.*, Vol. 28, pp. 241–49 (1916).

statements in his paper to which it seems worthy of calling attention.

He states:[1]

> In the footings of the different analyses it will be noted that the 12-hour digestions give footings more than 2.5 per cent. over 100. In the 15-hour digestion the footing is good, while in the 24- and 48-hour digestions the footings are 2.75 per cent. less than 100, thus indicating that the 12-hour experiments were probably not completely hydrolyzed; whereas the 15-hour digestion was sufficient to bring about complete hydrolysis and the 24- and 48-hour experiments were over-digested to the extent that nitrogen was lost.

One can but wonder where he secured such reasoning, or it is needless to point out that not more than 100 per cent. of the original nitrogen can be present in a protein hydrolysate even if complete hydrolysis has not taken place and the literature of proteins shows that no nitrogen is lost by overhydrolysis. Gortner and Holm[4] recently hydrolyzed fibrin for 6 weeks and obtained a recovery of 99 per cent., while 201 hours' hydrolysis showed a recovery of 100.7 per cent. the figures being, in both instances, within the experimental error of the analyses.

However, his most astounding conclusion is that the nitrogen in the insoluble residue obtained from the casein starch digestion "*is in an inert form and its estimation should not be included in the humin determination,*" with the result that he ignored the presence of nitrogen in this fraction in calculating his nitrogen distribution. Unfortunately he does not tell us how much nitrogen remained in this fraction[5] so that we can not recalculate his data, and as a result all of his laborious analyses are worthless. I use the word "astounding" in the above sentence advisedly, for in all of the protein literature I can find no reference to the black humin of protein hydrolysis which does not define it as insoluble, unreactive and inert, and any one who has studied its properties knows well that it is one of the least reactive of the chemical substances ordinarily met with, resembling in much of its behavior ordinary bone black. The humin of protein hydrolysis is a black, granular, non-crystalline substance, insoluble in the ordinary organic solvents, somewhat soluble in alkalis from which solution it is precipitated again by acids, and, in short, the true humin of protein hydrolysis agrees in every respect *as regards physical properties* with the material which McHargue discards and refuses to call humin.

Then again, the nitrogen of the fraction which he discards certainly belonged to the original casein, for his starch was practically nitrogen-free. How then can he claim that hydrolysis in the presence of starch does not alter the nitrogen distribution? This nitrogen which he discards belonged to the original protein molecule and should be included in the starch hydrolysates if it is included in the original casein analysis with which the starch hydrolysates are compared.

As I have shown previously (2), hydrolysis in the presence of carbohydrates causes a very considerable increase in the insoluble humin fraction and this increase is due to both chemical and physical causes. The nitrogen in the true humin of a protein hydrolysate has its origin almost wholly in the tryptophane molecule[6] and the reaction by which it is formed appears to be the condensation of tryptophane with an aldehyde or ketone. When carbohydrates are present the acid causes the formation of furfural which condenses with the tryptophane to form a "humin." However, furfural itself has the peculiar property of polymerizing(?), in the presence of 20 per cent. hydrochloric acid, to a black insoluble substance with the result that a large mass of porous black material is formed in the hydrolysate and this material, presumably through *physical* means, retains a very considerable amount of non-tryptophane nitrogen which normally would not appear in the humin fraction. *Perhaps* these latter forms of nitrogen would not be present in the black mass formed from the furfural in as great a quan-

[4] R. A. Gortner and G. E. Holm, *J. Amer. Chem. Soc.*, Vol. 39, pp. 2736–2745 (1917).

[5] He does give the per cent. of nitrogen in the black material but not the weight of the black material.

[6] R. A. Gortner and G. E. Holm, *J. Amer. Chem. Soc.*, Vol. 39, pp. 2477–2501 (1917).

tity in a twelve-hour hydrolysate as in a forty-eight-hour hydrolysate, but there is no question but that a part of the tryptophane nitrogen would be in this fraction.

It is of interest to note that McHargue obtained no "insoluble humin" for the twelve-hour hydrolysate of casein to which no carbohydrate had been added, and that his "histidine" fraction is in excess of that reported by other analysts. This observation accords beautifully with the idea of Gortner and Holm[6] that an aldehyde or ketone must be present to cause insoluble humin formation from tryptophane and that when insufficient aldehyde is present and the hydrolysis is not sufficiently prolonged the tryptophane will be (in part) precipitated by phosphotungstic acid and augment the "histidine" fraction [cf. Gortner and Holm[4]].

However, all of this discussion, pertinent as it may be, would be trivial were it not for the fact that other workers may be led to accept McHargue's conclusions and thus cause a further waste of money and energy in pursuing an illusive will-o'-the-wisp.

In the introduction to his paper McHargue seems to argue that Van Slyke's method may be applied directly to feeding stuffs without necessarily securing inaccurate results. Even if we should grant that the presence of carbohydrates *per se* did not vitiate the results, and all available evidence is contrary to such a conclusion, there would still remain other forms of nitrogen than proteins in the feeding stuffs which must necessarily appear in the various fractions and be wrongly calculated as amino acids. For example, Steenbock[7] reports the presence of stachydrin in alfalfa and this substance would be calculated as "histidine" in a Van Slyke analysis. I have elsewhere[2] fully discussed this point and therefore have no hesitation in making the following statements: (1) Proteins can not be hydrolyzed with 20 per cent. hydrochloric acid at atmospheric pressure in the presence of a considerable quanity of carbohydrates without appreciably altering certain of the nitrogen fractions of a Van Slyke analysis, and (2) a Van Slyke analysis applied to feeding stuffs, containing as they do non-protein nitrogenous compounds, gives no valid index as to the presence or absence of any individual amino-acid.

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[7] H. Steenbock, *Sci. Proc. Soc. Biol. Chem.*, XXVII., 1916; *J. Biol. Chem.*, Vol. 29 (1917).

THE ACADEMY OF SCIENCE OF ST. LOUIS

At a meeting held on May 20 Professor Francis E. Nipher stated that he had been making observations on local variations in the electrical potential of the earth, due to local thunderstorms. The large masses in the Cavendish apparatus are connected with a wire passing through a window in the second story of the physics building to the earth. The wire is in contact with wet grass in the yard below, and with metal rods which are pushed down into wet ground to a depth of about 15 inches. The lightning rod which grounds a high metal tower on the building, which was formerly used for wireless telegraphy, has been broken near the earth, and a gap of about two inches has been made in the rod. This rod can at any time be put in metallic contact with the large masses, by means of a knife switch. On several occasions during storms, sudden changes in the attraction of the large masses upon the suspended masses within the metallic shield have occurred, which it seems impossible to explain except as due to enormous changes in the potential of the large masses, due to local changes in the electrical potential of the earth. Previous results show that this would change the gravitational attraction between the masses.

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